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CHOLERA:

ITS

ÆTIOLOGY, CONTAGIOUSNESS,

AND

TREATMENT.

W. BOYD MUSHET, M.B.

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BY

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"Dira lues.
* * *
Sua cuique domus funesta videtur.
Et quia causa latet, locus est in crimine notus.
Semanimes errare viis, dum stare valebant
Aspiceres; flentes alios, terræque jacentes;
Lassaque versantes supremo lumina motu."—OVID.

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AUTHOR OF

'APOPLEXY (CEREBRAL HÆMORRHAGE): ITS PATHOLOGY, DIAGNOSIS, THERAPEUTICS, AND PROPHYLAXIS;'

'PATHOLOGICAL CONTRIBUTIONS TO MEDICAL JURISPRUDENCE;'

'THE PATHOLOGY OF ANGINA PECTORIS;'

'THE ÆTIOLOGY OF PULMONARY APOPLEXY;'

'NEW BRIGHTON: ITS SANITARY ASPECTS AND MEDICAL CLIMATOLOGY;'

'AN OBSOLETE MATERIA MEDICA;'

'THE AGE OF CLAY—A RHYTHMIC SATIRE;'

ETC., ETC.

PREFACE.

FEW of the present generation of medical practitioners have had opportunity of observing cases of Cholera. Amongst the majority, a tendency exists to entertain views *a priori* in favour of its contagiousness. This was amply manifested some years since during a debate on the subject at the Harveian Society of London. Men of wider experience, however, both in India and England, are inimical to the theory. Emphatic expression of this more matured opinion is important, by imparting confidence to nurses and others attendant upon the sick.

With regard to therapeutics, most is to be expected, in future visitations of cholera, from rational employment of the hypodermic method.

NEW BRIGHTON,
CHESHIRE.
August, 1883.

CHOLERA:

ITS ÆTIOLOGY, CONTAGIOUSNESS AND TREATMENT.

ÆTIOLOGY.

Of the intimate cause and essential nature of cholera we are yet ignorant; nor does clinical or post-mortem observation appear to render its pathology more definite or satisfactory.

We observe a train of phenomena varying in character and intensity in particular cases; but the study of symptoms during life does not enlighten us as to the seat of the disease, or aid us in the establishment of rational principles of treatment. In fatal cases we discern no anatomical lesion, and beyond more or less marked pulmonary collapse and venous congestion, there is exhibited no appreciable alteration of the viscera. The flocculent ingredient giving rise to the characteristic rice-water, or "congee" evacuations, which has been supposed to contain some entity having an influence on the causation of cholera, was long since pointed out by M. Boëhm, of Berlin, to be merely the rapidly shed epithelium of the alimentary canal, and Dr. Hassall's minute microscopical examination of the blood, urine, epidermis, and clothes of patients failed to detect anything unusual. In like manner, the fungi and vibriones detected in the atmosphere, air passages, alimentary tube and discharges, have been also found in the larynx and contents of the intestines of healthy persons by Dr. Hassall, Mr. Rainey, and other investigators.

But many other diseases are characterised by like obscurity of origin, like negation of evidence on autopsy, though they have not demanded so much attention, owing to the infrequency of their occurrence, or to their being less deadly, or

less rapidly fatal in progress. The stomach and intestines, liver, spleen, and other viscera have been respectively considered to be most implicated or concerned in the evolution of the disease, the organ in question being conceived to undergo some vital or functional perversion in consequence of the action of a specific *materies morbi*. Many, as Mr. Grove, have ascribed this to sporules or animalcules suspended in the atmosphere, or drinking-water, and Dr. Sansom has revived the opinion, as he holds that the cholera germ is *organised*; and that it *lives* and *grows* and *multiplies*. Mr. Grove maintained that to "*some form of life* (most probably vegetable) the principle of epidemic and infectious diseases is to be referred; that the predominant or governing influence is due to reproduction, and that it is during the exercise of the reproductive faculty of the poison germs that the phenomena of acute disease are manifested." Still more recently Dr. Déclat and other French physicians strongly support the "fungoid" theory; but, although this doctrine is attractive and plausible, it presents numerous fallacies, and is altogether undemonstrable, as I have already noticed. Others assign the disease to volcanic or electrical conditions, and deduce scientific inferences from the state of the atmosphere, which prove on analysis to be merely circumlocutory avowals of ignorance, though the meteorological observations of Mr. Glaisher—an epitome of which will be hereafter given—are of interest, as they hold out a hope, in the future, of throwing a light on the origin of the epidemic. Again, the disease has been deemed by some to be dependent upon malarious or miasmatic agency, to which the spleen is considered to be specially obnoxious. In support of this the spleen may undergo some functional change in cholera, as the blood presents considerable alterations (such as increase of its corpuscles and viscidity, and diminished and diffluent condition of its fibrin), over which fluid this organ is regarded to possess great and peculiar influence. According to Mr. Gray, the spleen regulates the *quantity* and *quality* of the blood; but after extirpation, which does not inconvenience the animal, the increase of blood is equally distributed over the whole system, so that the vessels contain more than under ordinary circumstances. Of course,

the viscidity of the blood is mainly due to separation of its watery constituents; but may not this viscidity be dependent (it is asked) on perverted action of the spleen failing to maintain hæmostatic equilibrium, and consequently favouring elimination of the thinner portions of the blood by the alimentary canal? If this theory be accepted, the analogy may be carried further, and cholera be regarded as a species of intermittent (Dr. Billing), the cold fit of which becomes developed, but it is not succeeded by the hot stage, in consequence of the concentrated or more subtle action of the malarious poison. The secondary fever, however, if collapse be recovered from, may be viewed as the analogue of the hot stage. Ague districts are said to be not particularly liable to visitations of cholera, though, if visited, the disease has been reported to rage with peculiar virulence; but such facts do not militate against the theory, as the diseases are presumed to be similar, not identical, in their mode of generation. But it is impossible to reconcile what has been observed relative to the origin and propagation of cholera with such a theory; there is complete absence of evidence in its favour; and lastly, the splenic derangement which is the *effect* of intermittents cannot be fairly, according to such a method of reasoning, considered, if it exist, as the *cause* of cholera.

Numerous authors maintain that the disease is *always* traceable to impurities in the water of the district; but this fails to explain its appearance or production under circumstances where the water has been avoided, or found free from noxious ingredients, and although there is great truth, there is probably not the whole truth in this view, as water only proves a source of cholera under certain conditions, which will be more fully alluded to. Even Dr. Snow, the great advocate of this theory, was unable to explain all circumstances connected with the visitation by the aid of the water theory alone; yet it must be admitted that in several instances, as during the memorable outbreak near Golden Square, in 1854, and in East London in 1866, the introduction of impure water into the system was almost mathematically demonstrated to be the chief, if not the sole agency in the development of the disease. Very many other examples might be quoted in proof of the

connection between impure water and the prevalence of cholera, and there is also truth, if not the whole truth, in the remark of the late Dr. Dundas Thomson, "that where cholera was most fatal in the metropolis the water supply was most impure." Nevertheless, concerning the water supply in relation to cholera, I hold the summary derived from evidence furnished to the General Board of Health in 1854 to be equally true—viz. "that there is no sufficient proof that impure water acts specifically in generating cholera, or, in other words, that it is the specific cause of the disease." This conclusion should carry great weight, when it is borne in mind that Drs. Hassall, Dundas Thomson, and other competent men were specially engaged in the investigation, and that Dr. Hassall concludes "that microscopical examination of the different waters obtained from houses visited by cholera—though denoting impurity as a characteristic feature—throws no special or direct light upon the production of the epidemic." To revert to earlier writers, Dr. Roupell, in his 'Croonian Lectures' for 1833, regards "the primary cause of cholera to be an affection of the nervous system, and that the immediate result of this is an irritability of the *lymphatic portion* of the vascular system, occasioning increased fluid discharges." He thought the discharges in cholera are similar to the contents of the lymphatic vessels, and that a local impression on the nervous centres of the abdomen which supply the viscera produce the symptoms of collapse without impairment of intellect, cold sweats, pulselessness, &c. Such a view was originally entertained by many Indian practitioners nearly half a century since, and Mr. Sedgwick likewise labours to prove that cholera is the result of functional disorder of the sympathetic nervous system, excited through the medium of the alimentary canal, chiefly of the stomach. Dr. Sansom also affirms that cholera is due to *irritation*, not *paralysis*, of the abdominal sympathetic.

Dr. Parkes, however, says, the leading idea he has formed of the nature of cholera is not only that it is primarily a disease of the blood, as has been generally surmised, but that the changes induced in the function of respiration directly consequent on the alteration of the blood are the proper and distinctive symptoms of the disease. The alterations of the

pulmonary functions, therefore, enter into his definition of true cholera, and for the purpose of expressing this decisively he has ventured to employ the term "algide cholera" as a synonym for "cholera gravior" of Orton and others. He thinks the cause of cholera to be a septic agent, or *materies morbi*, rather than a result of atmospheric changes.

Dr. Snow thought the disease due to some sort of structure, most likely a cell, though not to be recognised microscopically, any more than that of variola. He also held that the poison must be swallowed, or introduced into the alimentary canal, most frequently from water. Notwithstanding these views, he thought if the blood were poisoned the disease would be ushered in by febrile or other general symptoms. The diminished volume and thickened state of the blood accounted in his mind satisfactorily for all the symptoms of cholera, and he thought the blood was not poisoned, except in cases of consecutive fever. He adduced the fact of temporary restoration of the patient by diluting the blood with a weak saline solution as an additional proof that the circulating fluid is not poisoned in collapse.

Not to enlarge too much on this part of the subject, I will merely quote Mr. Headland (*Lancet*) in evidence of the contrariety of views entertained by different members of the profession. This gentleman thinks cholera is a blood disease, probably an affection of the corpuscles, as animal heat, &c., is impaired, which function physiologists attribute to the corpuscles; and, again, collapse may occur (he says) before thickening of the blood occurs from exudation of the liquor sanguinis. He is a non-contagionist, but thinks there is no connection between cholera and diarrhœa.

The most reasonable presumption appears to me to be that cholera is dependent on a septic animal poison, which is similar to the view held by the late Mr. Herapath, of Bristol. I do not conceive it essential that this poison be organised, or a specific cell, as odorous matters, such as musk, snuff, &c., though *particulate* in a high state of volatility, are neither gaseous, fungoid, nor animalcular, and yet they are decidedly capable of exerting physiological effects. It is a commonly received opinion amongst Continental physicians that *ochlesis*

and want of cleanliness will produce the disease—circumstances most favourable to the production and evolution of decomposing animal matters. Again, experience demonstrates that cholera appears in greatest virulence in localities where there exist cesspools, gullies, drains, foul sewers, poisoned water supply, and putrefactive exhalations. It is highly probable that emanations from these sources enter the system through the alimentary (or pulmonary?) mucous membrane. The effect of such septic matters is to produce disturbance of the functions of the gastro-intestinal tract and other phenomena of cholera, varying in degree according to the intensity or peculiarity of the poison; for as each province or district has its own source of decomposing materials, we must admit, I think, a plurality of choleraic poisons; and observation tends to confirm such doctrine, as at the same period of the same epidemic the disease appears to be milder in certain localities and certain individuals, for which habit and constitution alone fail to account. Under this head I may call attention to the diarrhœa and intestinal derangements often witnessed amongst students exposed to the putrid atmosphere of the dissecting-room, and to the production of serous diarrhœa in irritable subjects after indulgence in high game and other partially decomposed food, of which healthy persons partake with impunity, and obviate the effects by the antiseptic properties of their gastric and intestinal juices.

The septic poison produces a modification in the functions of the alimentary tract, manifesting itself in diarrhœa and vomiting, the symptoms differing much according to the potency and speciality of the operating cause and the constitutional powers of the patient, as the poison will of course act more energetically on those subjected to exhaustion, privation, or excess. I believe, then, that the causes constantly exist, and are in operation, which may produce diarrhœal symptoms, but why at particular intervals these symptoms should merge into cholera is at present a mere speculation, and may ever remain a mystery to man. The causes contributing to the production of cholera must have existed ever since mankind has been congregated in masses, although we have no authentic evidence of the appearance of the scourge until the present century. Mr. Curtis, surgeon in Sir Edward

Hughes' squadron, in 1782, gives a graphic account of a disdisease which, according to Dr. Roupell, was undoubtedly cholera, and from the description there cannot be much doubt of the correctness of the opinion. It may be found in the Madras Reports. Most authors, however, date its appearance at Jessore in 1817, and at Sunderland, in England, in 1831; but, from the foregoing, such a chronology is incorrect; and many hold that the black death of the middle ages and various epidemics of plague may have been true cholera visitations.

Meteorology.—Mr. Glaisher, in his Report on the Meteorology of London (Report of the Committee for Scientific Inquiries in Relation to the Cholera Epidemic of 1854), states that during the prevalence of the disease there was undue height of the barometer, the temperature was above the average, and there was less range of temperature than usual; the atmosphere was calm, still, almost stagnant, thick and *misty*—thin in high places, dense in low. The rainfall was unusually small, there was deficiency of positive electricity, and the ozone was very deficient, almost undetectable in low districts. These conditions are favourable to the accumulation and concentration of poisonous matters in the atmosphere, and of course augment the potency of their action. They depend on physical causes as much beyond the control of man as the variation in weather, temperature, and season, and either constitute or intensify the "epidemic influence." In 1866 Mr. Glaisher noticed the meteorological characters were different from those of former cholera periods. Thus, between midsummer and the end of the quarter, the atmospheric pressure was remarkably low, and the temperature of the air also low, night and day, except in September, when the nights were warm. The daily range of temperature was small, and there was abundance of rain everywhere. The air was in almost constant motion, frequently blowing much heavier than usual. Nearly all the circumstances were directly opposite to those mentioned as being present on previous visitations. They probably aided in checking its wider extension. One of the most remarkable atmospheric phenomena was the prevalence of a peculiar *blue mist*, which was generally present, extending from Aberdeen to the Isle of Wight, of the same blue tint

everywhere. This increased in intensity through the telescope and in density during the fall of rain. It did not decrease when the wind blew moderately, but did when a gale was blowing, increasing again on its subsidence. Mr. Glaisher does not know the nature of this mist, but it had not been noticed since the cholera period of 1854, which points to a possible connection. Dr. Leith Adams, however, disputes the relation between this blue mist and the epidemic, as he has observed it in almost every quarter of the globe when cholera was not known to exist within thousands of miles.

I assert that cholera must be regarded as diarrhœa maligna, or diarrhœa assuming a malignant type under certain almost unknown cosmical conditions, which can only be defined as "epidemic influence," "epidemic constitution," or, as M. Guérin prefers, "medical constitution;" though, with the exception of Mr. Glaisher's observations, we are utterly ignorant whether such factor be meteorological or telluric.

Most authorities agree that cholera cannot become epidemic except under certain conditions of the atmosphere. Dr. Gavin Milroy, Sir Thomas Watson, Dr. Bastian, and Professor Maclean admit an epidemic constitution. We can no more clearly explain why cholera should be, or be not epidemic, and why its epidemic features vary, than why typhus, small-pox, scarlet fever, measles, and influenza should be periodically epidemic, and alter so greatly in prevalence and severity. Why does scarlatina epidemically assume a malignant type? Why does it more greatly prevail at special seasons? Why did measles appear about the eighth century? And why do diseases at times assume a peculiar character, as during the "furunculoid" epidemic nearly two decades since? Scarlatina is always more or less rife, but is not malignant; diarrhœa always is more or less prevalent, but not cholera. Is there any pathognomonic or diagnostic feature, or line of demarcation, to distinguish diarrhœa from premonitory cholera? Diarrhœa graduates imperceptibly into the most malignant forms of cholera. Cholera may be sporadic, slightly or severely epidemic, or almost pandemic—analogous to other diseases which equally vary in intensity, but are specifically identical. Thus, measles may be *mild* or *black;* scarlatina

mere indisposition or a pestilence; smallpox may be discrete and *benignant*, or *malignant* and hæmorrhagic. Fever even may be comparatively mild, or accompanied with buboes and sloughing, even in England, and it may assume almost the characters of plague. Cholera and diarrhœa differ in degree, in intensity, but they must be held to be, in my opinion, fundamentally and pathologically the same disease. The profession indirectly confesses the identity of diarrhœa and cholera, as ordinary cases of diarrhœa are denominated English cholera, and in the *Lancet* (September, 1854) the necessity of calling every case of diarrhœa cholera is imperatively insisted on. The Committee of the Board of Health, also in the same year, considers that "when cholera and diarrhœa prevail together epidemically they are (with differences of degree) the same disease." In returns of deaths from cholera, deaths from diarrhœa are always included and placed side by side. Why is this if the affections are distinct, and diarrhœa is unconnected with cholera. Does not one practitioner call many of the severer forms of diarrhœa cholera, which another designates choleraic diarrhœa? It is said that cholera may, and does kill, without the supervention of diarrhœa, and that the latter is only a local pathological expression, or anatomical character, so to speak. Dr. Macloughlin believes such cases to be mythical, and they are, to say the least, infinitely rare, and there is no authentic case, I believe, where the stomach and intestines have been found free from excretions, and in which there has been absence of diarrhœa and vomiting from the attack to death.

The theory of a septic animal poison is in accordance with the origin and mode of propagation of the disease, as on what other hypothesis can we explain its appearance in distant parts after the visit of vessels from infected countries, or its development on shipboard far from any port? May it not, as M. Guérin believes, arise *de novo?* as it originally did in India; and may not the apparent importation be merely a coincidence, occurring at a time when the epidemic influence exists, and is capable of developing the disease in hitherto unaffected quarters? And when appearing in mid-ocean, is it not due to septic matters collected in the confined limits of the vessel,

and rendered active by the ship traversing in its passage the epidemic atmosphere which is passing from one continent to another.

Of course, those who affirm that the epidemic is always the result of importation, the product of contagion, or a special invisible zyme, sometimes dormant, sometimes active, equally affirm the absolute non-connection, non-identity of diarrhœa and cholera. With such, although the symptoms in cholera point most obviously to the intestinal tract, the disease is regarded to be the result of a constitutional poison manifesting itself by local effects; whilst diarrhœa is wholly referrible to local, not to systemic causes; and they explain the reason of diarrhœa assuming choleraic features by the fact of diseases being frequently modified by the prevailing epidemic, and assuming more or less of its impress and character. But I rather regard cholera as diarrhœa invested with peculiar malignancy from operation of an epidemic influence, as no division can be practically established between simple diarrhœa and the most virulent algide cholera during the existence of an epidemic.

Dr. Charlton Bastian ('Epidemic and Specific Contagious Diseases'), with whose views I coincide, believes that cholera, typhoid, typhus, plague, relapsing fever, influenza, cerebro-spinal meningitis, erysipelas, pyæmia, puerperal fever, glanders, hydrophobia, and syphilis may be generated *de novo*. He also believes that the several (contagious) specific diseases *may* have at times a spontaneous origin. He utterly rejects the "germ" theory, as there is no evidence of the presence of germs in the blood or other fluids of the body, and the theory demands a belief in the existence of about twenty different kinds of organisms, never known in their mature condition. He thinks epidemic diseases "are caused and propagated by chemico-physical agencies, and not by multiplication of living units."

Mr. Jonathan Hutchinson, too (*Brit. Med. Journal*, Feb. 7th, 1874), after an immense experience, believes that the several hospital plagues—viz., septicæmia, pyæmia, erysipelas, and hospital gangrene are frequently of autogenetic origin, though they subsequently spread by contagion.

During visitations of cholera, numerous cases of severe or choleraic diarrhœa occur and recover—many without treatment—not merging into collapse, explicable by the constitutional vigour of those affected, or by the mildness of the septic agency, or non-existence of sufficient epidemic influence, or a combination of several of these operations antagonistic to the development of the malignant form. It is said that there is greatly increased prevalence of diarrhœa at cholera periods; but are the cases, though doubtless more numerous, apparently augmented in consequence of public apprehension and more frequent application of patients for medical assistance? Cases wherein the algide symptoms appear to be almost unpreceded by premonitory diarrhœa are to be explained by the antecedents of the patient, as age, exhaustion, privation, &c., rendering him peculiarly prone to the reception of the poison, which, on such occasions, may be of profound malignity and intensity.

In a large proportion of cases, and even during a local outbreak of cholera, impurity in the water may be adequate to explain the propagation of the disease, but it is altogether inefficient to account for the origin, the *causa causans* of the pestilence. Water only proves a source of disease when loaded with animal impurities, under which circumstances it acts by introduction of septic materials into the system during the persistence of the epidemic influence.

With reference to this point, Dr. Sutherland observes, in his Report to the Board of Health in 1854, that "it is believed by some, that the water which induces cholera contains the specific poison of cholera in it, probably derived from the evacuations of cholera patients; while others believe there is no sufficient evidence of this being the case, and they consider all the facts go to prove that water, containing putrescent organic matter, acts as a very powerful predisposing cause of the pestilence, in a similar way as does putrescent organic matter introduced into the system by the atmosphere or by food, but not as a specific poison." In a like manner, recent disclosures advise us that vitiated milk may be a vehicle of septic poison.

According to such testimony, cholera requires *two* factors

for its generation—viz. septic material *plus* the epidemic influence. This latter, the epidemic influence, seems to be indispensable in the causation of the disease, as dirt, filth, and overcrowding are inoperative *per se*, otherwise cholera would be a constant, not an occasional scourge. On the contrary, zymotic diseases, though varying in prevalency, are never altogether absent from the community. This epidemic influence, physical inappreciable (?), appears to be only active in the vicinity of centres of putrefaction. It would appear to be a meteorological or atmospheric condition, itself variable in energy, even when existent, diffusible according to the direction of the winds, temperature, &c.

The disease is most rife, most malignant, in situations where there is greatest aggregation and evolution of septic animal matters, low level and stagnation of the atmosphere almost invariably contributing to intensify their operation. After a time, the epidemic influence may be dissipated or exhausted, but it appears to be capable of continuance or continued generation, as cholera is reported to be endemic in the Delta of the Ganges, and also amongst the Russian troops in the Caucasus.

The history of plagues and pestilences obviously establishes their intimate connection with the neglect of sanitary laws, and although there may be requisite a peculiar epidemic constitution of the body or of the atmosphere, disease is unproducible without the presence of filth, dirt, refuse, impure water, organic decomposition, or neglect, defiance, or violation of hygienic measures. Dr. Roupell laid great stress on animal effluvia, defective sewerage, and intramural cemeteries, and Dr. Parkes says, that "cholera is most violent and common in the dirtiest and densest parts of the town, less severe amongst the English soldiers, and not at all seen in the *houses* of the English residents *scattered* about." Mr. Grove concurs that diseases amongst the poor owe much of their inveteracy to the accumulation of effete organic matters about their clothes and persons.

The septic poison of cholera, which is sufficient to produce every variety or degree of symptoms, from simple diarrhœa to overwhelming collapse, does not materially interfere with the

sensorium or the muscular apparatus, inasmuch as there is no impairment of the vital integrity, no specific poisoning of the blood. There is no exaltation or exaggeration of nutrition, no increased disintegration of tissue or organic change, as in fever, but *a diminution down to a total arrest of the nutritive operations;* so that there are no deleterious elements in the circulation. At the outset, the irritation of putrescent matters in the alimentary canal is most probably local, accompanied by more or less profuse shedding of epithelium; though later, it is likely that an impression is exerted on the sympathetic nerve of the abdomen, which may secondarily become involved. Dr. Carpenter admits that nutrition, secretion, and other vital processes may take place independent of nervous influence, and the enormous surface of the denuded intestines may passively allow the copious transudation which ensues. Still, the nerves greatly influence secretion, and it is a remarkable fact that notwithstanding the blood is universally dark in the vessels, I distinctly noted, in two cases of abortion in which the subjects died during collapse, that the hæmorrhage from the uterus was arterial, bright red in colour. The functions of the placenta, which are very active and important, are performed, it will be remembered, entirely without the assistance of the nervous system.

I now pause to discuss categorically the widely disseminated doctrines of Dr. George Johnson, who occupies a most conspicuous position amongst recent writers on Cholera. It is essential, therefore, to be somewhat discursive, and afford a special *résumé* of the views of this physician, with the arguments which may warrantably be urged against the soundness of his conclusions.

According to Dr. Johnson, cholera is the result of a material and portable poison, capable of conveyance from place to place, and of communication from person to person. It may enter through the lungs or alimentary canal.

Water is a main vehicle by which the poison enters the system.

It is absorbed into the blood.

The period of incubation is usually from one to three or four days.

The discharges are a conservative effort to expel the poison and its products, analogous to the eruption of smallpox and the diarrhœa of typhoid fever.

The discharges, when abundant, may be so copious as to kill.

They bear, however, no direct relation to the degree of collapse, and they are not the essential or chief cause of collapse, as collapse and death may occur without discharges, and with but scanty secretion into the bowels.

There is impeded circulation through the lungs, as the pulse is small or absent; there is fulness of the veins and lividity, and, after death, the left side of the heart is empty; the right side, the pulmonary artery, and the large veins distended; the lungs anæmic, light in weight. The explanation is, there is contraction of the minute pulmonary arteries—a primary asphyxia, a secondary apnœa. Dr. Johnson believes the cause of contraction of the walls of the minute pulmonary arteries is the poison in the blood, which acts as an irritant upon the muscular coat of the vessels, and the relative deficiency of carbonic acid in the blood he explains by the fact that the small current of blood through the pulmonary capillaries involves a correspondingly diminished formation of carbonic acid.

Now, with regard to Dr. Johnson's first proposition, which is an adoption of the prior views of Dr. Baly and Sir Wm. Gull, no evidence is adduced in support of the assumption of the materiality and portability of the cholera poison, and of its communicability from person to person. These hypotheses *may* be correct, but they prove nothing as to the specificity and contagiousness of the poison of cholera, and they are directly at issue with the belief of some of the highest authorities on the subject. As to the proposition that the poison is absorbed into the blood, similar objections will apply to those raised in reference to the preceding; but this will be more clearly considered in the sequel.

Dr. Johnson's proposition that the discharges are a conservative effort, similar to the eruption of variola, and the diarrhœa of typhoid, is founded on on a most erroneous analogy; as, if there be any resemblance in the morbid processes, we

should promote the eruption of smallpox by heat and diaphoretics, as eliminants, and administer cathartics in typhoid for a like purpose. Again, would the most strenuous attempts at elimination cut short the two latter diseases, *i. e.* arrest their further course?

If we admit, with Dr. Johnson, that the cholera poison is as drastic a purgative as any in the materia medica, and that this "cholora-cathartine" must and will purge itself away—is it therefore to be aided by further purgative efforts? As well may we attempt to treat a case of excessive evacuation from elaterium with castor oil, instead of striving to control the discharges by opium, sedatives, and astringents. But Dr. Johnson will doubtless reply that, in cholera we have "zymotic" changes in the blood, and not after a dose of elaterium; but I maintain that the presence of such changes is equally problematical in the former as in the latter, and that it is less rational to expose the patient to an undisputed *hypercatharsis* than to the *risks of a highly questionable zymosis.*

Dr. Johnson allows that the discharges may be so copious as to kill. This is admitted even by Dr. Johnson, who yet declares that the discharges bear no direct relation to the degree of collapse, that they are not the essential or chief cause of this, as collapse and death may occur without discharges, and with but scanty secretion into the bowels. Such cases are very, very rare, to say the least, even in India (see *Lancet*, June 2nd, 1866), as Drs. Parkes and Goodeve never witnessed a fatal case of cholera unattended with vomiting and purging, though, it should be added, that Dr. Parkes regards them as usual, but not essential symptoms, and thinks it probable that cholera might occur entirely divested of these symptoms. In England such cases are even more rare (has Dr. Johnson ever witnessed one?), and almost every one who has seen much of cholera admits that the evacuations are ordidinarily in direct ratio to the collapse. Thus, of 1798 cases of collapse reported to the Board of Health in 1854, all but 6 were attended by intestinal discharges, and in these exceptional instances it is stated that more accurate inquiry would most probably have elicited the fact that some intestinal flux occurred before the phenomena of collapse were developed.

Physicians seem to have overlooked to some extent the variations in constitutional power and in the previous state of health of persons attacked, consideration of which would in most instances explain apparent anomalies. It is well known that some few persons will become collapsed, and almost succumb to two or three ordinary diarrhœal evacuations, on account of feeble vital power, inherent debility, nervous idiosyncrasy, or existing cardiac disease. Such persons, it may be said, live on sufferance, requiring little disturbance to snap the thread of life. How unequally do different individuals bear operations, chloroform, pain, and other mental and physical shocks, many of whom, previous to the fatal seizure, have manifested no marked peculiarities, and provoked no particular medical attention! Now and then a patient will die from mere fright, the sting of a wasp, a crushed finger, or faint from the slightest causes.

Moreover, in these cases of "cholera sicca," did Dr. Johnson, even perform an autopsy, or hear of one having been performed, in which there was neither vomiting nor purging from the commencement to the termination of the case, and in which the intestinal tube was devoid of exhalation?

Dr. Johnson not only disregards the dangers of the premonitory diarrhœa, but promotes the symptoms by castor oil. It is not too much to say that, in cholera periods, a person with slight relaxation of the bowels is, in the opinion of Dr. Johnson, in a more desperate condition than a patient actively *ανω και κατω* evacuans. And yet, with singular inconsistency, this physician allows that the "abundance of the gastro-intestinal excretion is a pretty accurate measure of the severity of the attack"—the excretion which he is desirous of aiding and augmenting in defiance of its physio-pathological significancy. Again, he says, "no cases are more hopeless than those in which there is not only a cessation of vomiting or purging, but an almost complete arrest of the process of excretion from the gastro-intestinal canal." That is, that the patient has least chance where the drain has been so abundant as to to leave nothing more to be exhaled, thereby confessing that the choleraic discharges tend in proportion to their severity to induce irretrievable collapse.

Dr. Johnson's explanation of the phenomena of cholera is that there is a spasm or contraction of the minute pulmonary arteries, caused by a morbid poison in the blood, which acts as an irritant to the muscular coat of the vessels. Here Dr. Johnson argues from facts to hypotheses, as the lividity, fall of temperature, and purgation can be otherwise better explained; and the imaginary morbid poison is a sort of *Deus ex machinâ* introduced to account conveniently, but imperfectly, for the arterial spasm, which is limited, as far as can be discerned, to the pulmonary vascular radicles, though the phenomena point to impeded circulation in every part and organ.

We search in vain for enlightenment why the pulmonary vessels are alone subject to the irritant action of the oft-invoked poison. This view of muscular contraction, spasm, obstacle, difficulty or thickening appears to be a hobby of Dr. Johnson, as he applies it to the explanation of Bright's disease, apoplexy, epilepsy, and cholera. Indeed, he defines cholera collapse as pulmonary epilepsy! (*British Medical Journal*, March 21st, 1868). This is a revival of the pathology of Cullen, a recurrence to spasm of the vessels ingeniously spun out to a most sophistical tenuity. As a sample that these observations are not unfounded, Dr. Johnson, in a communication to the *Medical Times and Gazette* on July 2nd, 1870, as to the proximate cause of hæmorrhage into the brain in chronic Bright's disease, maintains that it is due to "hypertrophy of the left ventricle, resulting from excessive contraction of the minute systemic arteries, which impede the onward movement of the blood, thus calling for increased efforts on the part of the heart to carry on the circulation. The muscular walls of the minute arteries are much hypertrophied as a physiological result of long-continued overaction. The struggle between excessive cardiac force and excessive arterial resistance increases the risk of hæmorrhage consequent on rupture of the minute arteries." This dictum is, nevertheless, controverted by a quoted case in which the "arteries of the pia mater presented no appearance of hypertrophy;" and Dr. Johnson thinks it probable that in this instance the cerebral hæmorrhage resulted from rupture of the capillaries, and that the immediate cause of this rupture was excessive pres-

sure on the cerebral capillaries, due to the fact that the propelling force of the hypertrophied left ventricle was *not* counter-balanced by an equivalent hypertrophy and consequent resisting power in the minute cerebral arteries; or, in other words, that the hypertrophy of the minute cerebral arteries, if present, is held to account for the hæmorrhage, and when absent, is equally called upon to explain its occurrence, to the exclusion of atheroma and degenerative changes. Dr. Johnson has observed in cases of chronic Bright's disease, with hypertrophy of the left ventricle, that, as a rule, the minute arteries in all the tissues examined, as kidneys, pia mater, subcutaneous tissue, muscles, mucous membrane of the bowels, have their muscular walls hypertrophied. Yet hæmmorrhages into the kidneys, pia mater, subcutaneous tissue, muscles, and intestinal mucous membrane are singularly rare.

In cholera the spasm is most intense, the contraction of vessels greatest after the most evacuation, where there is, according to Dr. Johnson, the least poison remaining in the system. He thinks the collapse due to the contraction of the pulmonary arterioles, and that thickening of the blood is not the cause, but rather the consequence of the collapse. But it is in greater agreement with physiological truths to believe that the collapse, failure of circulation, and inspissation of the fluids are in direct proportion to the discharges and to the *arrestment of nutrition.*

There is, says Dr. Johnson, a primary asphyxia, a secondary apnœa; but, if asphyxia, why not coma, which is invariably the result of ordinary interference with respiration? And, as he explains the absence of syncope in cholera by supposing a venous turgescence of the brain, his difficulties increase in endeavouring to account for the non-occurrence of comatose symptoms. In other words, Dr. Johnson tells us that there is a condition of asphyxia, although the brain is clear, and the freedom from coma depends upon a primary asphyxia *plus* a venous turgescence of the brain! Dr. Johnson entirely overlooks physiological, nutritional considerations, and makes cholera to be due to non-absorption of oxygen by the lungs. His doctrines are mechanical, to the exclusion of chemical, of vital changes. It appears to be a more satisfactory theory than

that of pulmonary arterial contraction to ascribe the phenomena to *failure and gradual arrest of the nutritive forces*, however much we may differ as to the cause by which the effect is accomplished. According to Dr. Parkes, "patients die because the blood does not pass through the lungs, produced by some aberration of, or impediment to, the proper respiratory changes." In short, there is no nutrition, therefore no carbonic acid formed, and hence no necessity for respiration. Dr. Parkes truly said before Dr. Johnson that we must look to the *blood itself* for arrest of the pulmonary circulation. The blood is no doubt equally arrested in the capillaries of other parts of the body, owing to the *arrest of nutrition*; there is, in consequence, little or no need or power of circulation, little or no formation or evolution of carbonic acid, and therefore no requirement for oxygen and respiration. Parkes has noticed collapse of the lung in cases of consecutive fever in post-mortem examinations. Such appearances might be reasonably expected, as occasional, like the atelectasis of the newly-born if weakly, or the pulmonary collapse witnessed after fatal cases of pertussis, where the child is too feeble to expand the lung after violent expulsion of the residual air. In these cases of consecutive fever the appearances are the result of the algide stage, expansion not occurring after reaction in consequence of the debility of the patient.

The diminution in temperature, the shallow respiration, the mahogany blueness of the surface must depend upon deficiency of oxygen and of oxidation in the system. There is no *besoin de respirer*, as there is little or no chemical change—no carbonic acid is formed in the extreme parts, and we know the respiratory function or necessity is merely for the purpose of elimination and interchanging carbonic acid for oxygen in the lungs. We also know that the temperature is proportionate to the activity of the nutritive processes. Hence the shallow breathing—as there is no carbonic acid to exhale, no oxygen to absorb—the decreased animal heat, the darkened blood, which last, on account of the discharges, gradually becomes inspissated and stagnant from non-aëration,—devitalisation. Hence, also, the collapse of the lung, in many cases, from imperfect expansion, which also occurs, as I have stated, in young

children, as a result of pertussis and inability to draw in air for complete reinflation. Hence, also, the systemic collapse, the suppression of urine—as there is no disintegration of tissue —and of bile also in most instances. There is absence of sensorial disturbance, as there is no poison in the blood, no effete material, no formation of carbonic acid, to complicate matters; but there is an apathy, an unconcern in the algide stage, as we might expect, as a consequence of cerebral centres imperfectly supplied with blood, circulating languidly in the vessels. But patients can speak feebly; and are usually conscious up to the period of dissolution.

Again, there is not extreme exhaustion in cholera until the last, as venous or imperfectly (oxygenated) arterialised blood stimulates the muscles (Brown-Séquard). The heart, therefore, as a muscle, acts tolerably well, as does the general locomotive system, even when the patient is cold, haggard, with sunken eyes, Hippocratic face, and corrugated skin, which train of symptoms last enumerated depends in a measure upon withdrawal of fluid from the vessels by the discharges. On this account partly, as well as from diminished blood current —no carbonic acid seeking egress—are noticed the emptiness, dryness, and lightness of the lungs of those dying in collapse. The change, if present, is only physical, not vital nor causative. Little or no blood is sent through the lungs, as little or no carbonic acid is formed in the general capillaries. Probably other organs weigh less—though the lungs may be expected to show this most markedly—as the whole body must be lighter in weight if drained of its fluids, and if the whole body each or most of its parts.

The cramps, which are very variable in intensity in cholera, are due, says Dr. Johnson, to the irritant action of morbid blood, which excites spasm of the muscles; but I regard them to depend on insufficiently nourished nervous and muscular filaments, which differ much in irritability, according to the neurotic predisposition of the patient. Cramps were not so frequent or intense at the latter part as at the commencement of the epidemic of 1854. The presence of a poison in the blood in cholera, upheld by Dr. Johnson and other authors, can with difficulty be reconciled with the fact of a patient cold

and pulseless, in a state of collapse, becoming, without further evacuation upwards or downwards, in a few minutes, warm, naturally coloured, and rapidly convalescent, as I have witnessed, after a dose or two of sulphuric acid.

That saline injections act by relieving spasm of the vessels, as Dr. Johnson asserts, I believe to be altogether an error. He may as well call all passive congestion minute arterial spasm. Injections in cholera cause dilution and liquefaction of the inspissated blood, and, if hot, temporarily restore the circulation and animal heat, which last is radically interfered with by suppression of the organic operations; but their good effects are transitory, and cease as the temperature declines, as injections, says Dr. Parkes, never saved a case.

In reaction, Dr. Johnson holds that the drowsiness results from carbonic acid in the blood; but it is more probably due to re-formation of effete materials not excreted, as urea, &c. It is, in fact, uræmic, not asphyxial.

To sum up, I regard cholera to be a malignant form of diarrhœa, essentially identical in its nature, arising from a local trophic perversion of the alimentary canal, in consequence of the introduction of putrescent animal particles into its interior (occasionally through the lungs?), which, under some inscrutable influence or epidemic constitution, destroy the balance of the capillary circulation of the stomach and intestines, causing exhalation from their surface, attended by characteristic symptoms, one of the most marked and constant being, in the stage of collapse, suppression of urine. The general symptoms of cholera are, almost or complete pulselessness, haggard visage, eyes sunken, with striking areolæ, features pinched, lips livid, voice whispering, surface more or less mahogany blue in tint and cool, extremities cold, nails purplish or black, skin corrugated, tongue variable (may be clean), cramps in legs and abdomen (may be absent), great distress and restlessness (variable), great thirst, gruelly or rice-water evacuations, vomiting as if the whole stomach was abruptly emptied without effort, succeeded by retching, frequent moaning, and suppression of urine. In rare cases the dejecta are bilious throughout in the fatal form of the disease, but anuria is constant, and it is evident that the diminution of the renal

secretion is directly proportionate to the severity of the collapse, that is, to the completeness of the arrest of nutrition and of the organic functions. No urea is formed, and no urine is secreted, as the kidneys lack their accustomed stimulus. When consecutive fever supervenes, with restored tissue metamorphosis, there is difficulty of varying grade in re-establishing the renal function, and the urine is always albuminous. Hence there is a disposition to uræmia; in collapse, anuria; in reaction, ischuria.

The consecutive fever, however, is frequently out of proportion to the intensity of the antecedent collapse. It is at times severe, even after choleraic diarrhœa. I believe the consecutive symptoms depend much on their severity whether the stage of reaction has been of gradual or sudden invasion, on the mode of treatment adopted, and also on the previous condition of the kidneys, though other influences may contribute. Although the mammary secretion is not arrested in malignant cases, as Dr. Johnson has pointed out, *it is rendered serous.* I noticed this in a severe, but not one of the severest cases.

Do children under two years ever exhibit the characteristic (cyanotic) symptoms of cholera? I have observed several cases of diarrhœa of unusual severity in infants, which proved fatal during the epidemic, whose mothers had died from the disease, but the type did not justify the appellation of malignant cholera. I may add that the disease did not spread from these to other children in the nursery.

Dr. Ayre held that cramps were also invariably absent from children, and certainly, according to my own observations, they do not present the characteristic symptoms remarked in adults.

Contagiousness.

With regard to the contagiousness of cholera, there is a remarkable diversity of opinions. It is, of course, exceedingly difficult to prove a negative, that is, that it is *not* contagious. On the other hand, it is almost as easy to conclude affirmatively, when an outbreak occurs, and a large number of the

population is attacked within a limited area, often on most insufficient grounds, as in the case of the Broad Street pump, the water from which almost simultaneously affected several hundred persons. It is also significant that those who are most conversant with the disease in England are either non-contagionists, or regard it to be but mildly contagious, whilst those who have had little or no experience of cholera are firmly persuaded of its contagiousness, as were for the most part our predecessors, when panic-stricken by its appearance at Sunderland in 1831. Thus, in 1832, the College of Physicians affirmed the disease to be contagious, whilst the Committee for Scientific Inquiries in 1854 were undecided on the point. It may also be noticed that the views of authors as to contagion are often influenced by their special theories concerning the disease. Amongst old Indian medical officers, Annesley and Jameson were non-contagionists, whilst Orton was inclined to contrary views. Dr. Roupell believed cholera to be contagious, but he admitted that it may have had more than one source, and appeared in many places independently of transmission or contagion. He owned that "military cordons, and most rigorous continental enactments have proved inefficient in preventing the spread of the disease from infected to uninfected countries," which is an argument rather against its propagation by contagion.

Dr. Parkes, on the contrary, from his observations in India, does not consider the disease contagious. "He has never observed any indication of contagion." He says that communication with infected persons or districts did not cause the disease. He has inoculated himself accidentally in post-mortem examinations, and never suffered; nor did the hospital attendants or medical officers. He does not, however, wish to generalise his observation, and to conclude that the poison of cholera is never reproduced in the human body; but he has never witnessed any facts which led him to believe that the poison of cholera possessed the power of multiplying itself by its action on the living human system.

Dr. Copland believed pestilential cholera to be a specific disease, distinct from the severe forms of common cholera. He deemed it infectious, and essentially independent of epi-

demic influence, or of any particular state of the atmosphere, although it may be intensified thereby.

Sir Thomas Watson, Dr. Bryson, and Dr. George Johnson, all appear to be contagionists, that is, hold that cholera is a portable poison, capable of conveyance from place to place, and of communication from person to person; and Dr. Bryson asserts that "its spontaneous origin far away from an infected locality is unknown in the naval service."

Professor Maclean is evidently a contagionist, and Inspector-General John Murray, of the Indian service, thinks it to some extent contagious or communicable, but that there is not *very* great danger in attending on the sick in Great Britain, where decomposition is comparatively less rapid than in India. It may be remarked that Dr. Murray states he has suffered from three attacks of cholera, and this testimony is opposed to its being an acute specific disease, which usually occurs but once to the individual.

Dr. Snow, who threw so much light on the ætiology of the epidemic, also thought it was contagious or communicable from person to person.

Dr. Gavin Milroy thinks contagion plays a very small and subordinate part in the diffusion of cholera, and that it does not admit of doubt that it may arise *de novo*, and make its appearance at intervals in Hindostan quite independently of any suspicion of antecedent importation. Dr. Bastian holds similar opinions.

Dr. Cuningham, Sanitary Commissioner with the Government in India, in consequence of facts especially collected, arrives at the conclusion that the evidence is entirely against the contagiousness of cholera. He denies that it is a specific poison capable of multiplication in the bodies of those affected, or that it can be transmitted through the medium of water, or that human locomotion has anything to do with its propagation from place to place. Dr. Cuningham deems air to be the only agent worthy of serious notice in the propagation of the disease, and insists emphatically on a pure water supply, and on removal from infected districts—*i.e.*, withdrawal from the conditions with which the pestilence is associated. ('Report on the Cholera Epidemic in Northern India,' by Dr. J. N. Cuning-

ham.) It is probable that the disagreement noticeable amongst the authorities quoted might be much reconciled by a precise definition of the word "contagion," which has been far from strictly employed, and is, with many, convertible with "infection." "Words," says Max Müller ('Lectures on Language,' 2nd series, page 256), "without definite meanings are at the bottom of nearly all our philosophical and religious controversies, and even the so-called exact sciences have frequently been led astray by the same Siren voice."

I will now record my own experience during the epidemic of 1854, as to the contagiousness of the disease, which is highly confirmatory of the observations of Drs. Parkes and Cuningham; and I am fully justified in stating that my lamented colleague, Dr. Filliter, entertained similar convictions.

Neither doctors, nurses, helpers, nor undertakers, who were constantly in contact with cholera patients, alive and dead, were attacked by the disease. The undertakers passed many hours in the dead-house daily, and I have seen them taking their meals there, as under ordinary circumstances. The medical officers were in the wards at all hours of the day, and I have passed as many as four hours daily in the cholera ward, which was specially set apart for the reception of patients, and was full, too full, for a long period. One of the helpers, named Strong, was on duty all day, and slept every night in one corner of the ward, that she might be ready in case of urgency.

A patient on the same gallery, but in an ordinary female ward, visited from curiosity the ward in which the cholera patients were located. This woman was attacked, being the only instance which occurred amongst the patients in this portion of the infirmary. She had been in for some weeks, having ulceration of the leg, and expressed great apprehension of cholera, after having witnessed the patients who were brought in. In consequence she was removed from the surgical ward, farther down the gallery to a medical ward, at the request of the nurse, and ordinary diarrhœa supervened some days after her removal. This continued for two or three days previous to choleraic symptoms. She remained untreated, avoiding to inform the doctor, "lest she should be sent to the cholera ward." Death took place eight hours after the access of

characteristic symptons, although I saw her directly, when summoned. Concerning this case, it may be fairly held that the exhausting operation of ulceration of the leg, great moral dread and depression and continued diarrhœa rendered her peculiarly favourable to the reception of morbid impressions, combined with which there existed a foul water-closet, which was much complained of by the patients in the ward, situated close to it, and the septic emanations from this source may be regarded as the cause of the attack, without resorting to the aid of contagion as an explanation. The epidemic influence may be also presumed to have been present, as cholera was at the time prevalent in a court immediately behind the infirmary buildings. Two more cases, on the male side, afterwards appeared in another ward of the infirmary, far away from the situation of the female wards—the first, an old man, suffering from chronic vesical disease; and the other, the night nurse in the same ward. Both of these had previously had ordinary diarrhœa, and were feeble, elderly persons, subjected to a close, ill-ventilated surgical ward, with an offensive drain in one corner, and the above court immediately in the rear. The old woman recovered.

Two cases occurred in the workhouse of St. Marylebone—one, a nurse, not attendant on cholera patients; the other, a man employed about the beds, and who possibly had been in contact with the bed-clothes and bedding of the cholera patients; but several other men were almost equally exposed, without any development of choleraic symptoms. I refer to, although I attach no importance to, the consideration of exposure in the above instance. The nurse mentioned had had diarrhœa for many weeks, and was a confirmed drunkard, living in a most confined part of the workhouse, poisoned by noxious influences, and not very far from the court formerly alluded to. The bed-man was of irregular habits, had suffered for some time from diarrhœa, and was living under conditions of atmosphere similar to the nurse.

These were the only cases which occurred amongst the inmates of the infirmary and workhouse, and in all these three there was premonitory diarrhœa traceable to septic sources. There was also strong presumption of epidemic influence from

the known existence of the disease in the neighbourhood, and only one of the three could be at all connected with direct exposure to contagion. I attribute the immunity enjoyed by the inmates generally to the thorough steps adopted for the establishment of cleanliness. From the cholera wards the evacuations were removed by the nurses as soon as discharged by the patients, which I regard to be a most important prophylactic measure in respect of the attendants. According to tradition, in a previous epidemic, twelve or thirteen cases appeared in one ward of the workhouse which abutted on the churchyard in Paddington Street, the remainder of the house remaining free from attack. My conclusion, therefore, is, that cholera, literally speaking, is not contagious; and when apparently so from communication with the sick, it is generally or invariably due to subjection to the same circumstances which originally infected the first individual; or to exposure to decomposing choleraic discharges from the person or garments of those suffering from the disease, as in the case of the woman at Malta (adverted to by Inspector-General Anderson), who developed the complaint after (dependent on?) wearing the unwashed linen of a patient. I do not regard the evacuations in cholera to be specific as a poison, but rather causing the disease—if they can do so—as might any other virulent septic material; and the choleraic dejecta may be assumed as highly prone to decomposition. I am convinced that mere contact of the body, or the breath of a patient suffering from cholera, cannot induce the complaint. The clothing of cholera patients should be disinfected or destroyed, as an assurance of safety. The limited contagiousness, if any, of cholera should be insisted on, and published to all concerned in its treatment, being of great importance to those attacked, as it tends to impart confidence to nurses and attendants, and thus render them more assiduous to the sick. Patients, if possible, should be removed from infected districts to other localities, as they are withdrawn from the influence of the original cause and are more favourably circumstanced for treatment. Dr. Cuningham insists on the value of movement as a preventive of cholera, as facts point to a peculiar localisation of the disease.

On the whole, I consider the susceptibility of persons in

health to cholera as very limited, unless the poison or influence be of peculiarly malignant character, or there has been some previous privation, exhaustion, or indiscretion, as long fasting, over-fatigue, intemperance in food or drink; and if the history of cases amongst the better classes was ascertained and analysed, it would be in nearly all cases discovered that the attack follows bodily excess: and this, if the fact, explains the immunity of individuals in greatly infected districts. It is remarkable that during epidemics of cholera less deaths are noted on Saturdays and Sundays, owing to (?) freedom from depression, as there is rest from work, better food—wages having been paid—less water, and more stimulants taken. Galen observes, with truth, that epidemic influences, however powerful, are insufficient to induce disease without a peculiar disposition of the body to admit them; otherwise all, without exception, would be seized by the prevailing epidemic.

I have, to some extent, proved how rarely hospital attendants are attacked in the performance of their duties. If they are, they at first suffer from diarrhœa, which becomes malignant (cholera) from the predisposing nature of their office, confinement, irregular habits, watching, fasting, exposure to decomposing discharges and foul clothing of patients, and other sources of exhaustion, as well as sometimes to local epidemic influence. It is, therefore, not difficult to explain the causation, when doctors, nurses, and attendants are attacked, without seeking to maintain the contagiousness of the disease, *i.e.*, its communication by contact with those affected. These remarks closely coincide with the views of Mr. Whitfield, Resident Medical Officer of St. Thomas's Hospital, communicated to the Board of Health in respect of the epidemic of 1854, and with the more recent conclusions of Dr. Cuningham.

Moreover, the disease appears at sea in ships far removed from every source of contagion, and it is frequently arrested on the confines of an infected territory which is in free communication with surrounding uninfected countries. Lastly, that cholera should appear to travel, or be conveyed, in the lines of human traffic and intercourse, might be expected, without resorting to the theory of contagion, as it is only

amongst the human race that the conditions probably exist necessary or essential for its production.

Pathological Appearances.

In Dr. Parkes' forty-seven cases, *all had vomiting and purging* previous to admission into hospital. Cramps were not universally observed. There were diminution of animal heat, laborious respiration, and embarrassed action of the heart. There was no ratio between the vomiting and purging and the algide symptoms, and cases were often more malignant and more rapidly fatal with little vomiting and purging. The skin often became lighter after death, and the shrivelled appearance of hands and feet diminished. The most marked appearances in the head were venous congestion. *The lungs were diminished in weight,* from 17 to 20 ozs., *taking European lungs as a standard.* They were collapsed, and deficient in crepitation, pale or dark, becoming lighter by exposure. Right heart filled, often distended with blood; left heart generally empty, or almost so; blood generally fluid, sometimes coagulated, dark or fibrinous; no spasm of ductus communis choledochus. *Small* intestines *in every case* contained more or less fluid, which consisted of flocculi and a serous portion, consisting of water, salts, and a protein compound. Urinary bladder in every case contracted. In the lungs there was remarkable want of air. In the most rapid cases, also, a want of blood in the minute texture, causing the lung to collapse when the chest was opened, and its weight to be diminished in a very great degree. Gorged and congested lungs occasionally happened; yet in some of the worst cases there was a singular deficiency of blood in the pulmonary texture. Appearances therefore satisfactorily prove that there is more or less complete failure in the transmission of blood through the lungs; but this is not due to *failure of the heart,* as it beats in many cases till stopped by want of blood on the left side. Moreover, the respiratory muscles are not paralysed, and the lungs can be inflated readily after death, or the patient can breathe deeply during life if told to do so. We must look, therefore, to the *blood itself* for arrest of the cir-

culation through the lungs. The exuded fluids in cholera occur not apparently as secretions, but as exhalations, *i.e.*, they are not subjected to the vital influences of cells—true secretion seems arrested. Parkes considers the cold stage the true stage, and not the initiatory one, as in ague.

In twelve carefully reported post-mortems, recorded by Mr. G. G. W. Callender, Registrar of St. Bartholomew's Hospital (Committee for Scientific Inquiries, 1854, Appendix), most of the patients dying during collapse, the rigor mortis was well marked in all, the left side of the heart contracted, the right flabby; the veins (venæ cavæ, &c.) gorged with fluid blood or soft clots, the aorta and pulmonary arteries mostly containing soft clots, or, in some cases, fluid blood. In the aorta, small in quantity. In most subjects the kidneys natural, or pale. Capillary congestion of lungs and bronchi in most cases. Mucous membrane of intestines in some cases congested, in some pale. *Lungs in some cases natural or œdematous; in two or three, red in colour, and more or less carnified.* Peyer's patches in most cases natural. Liver pale or natural. Gall bladder in most cases contained much bile. Spleen natural. Bladder contracted, and empty in all the cases which died in collapse. Brain and membranes natural in every case, and a small quantity of serum in the ventricles of the brain.

On referring to my own notes of autopsies performed on adults and children dying during collapse, I find that the whole intestinal tract may be as pale, if not paler, than in the normal condition, and that the liver, spleen, and kidneys have been found not markedly congested. I have never seen spasm of the ductus communis. *The lungs have been at times congested more than any other organ*, and I have not noticed the marked pulmonary collapse spoken of by other authors. The most striking appearance has been the *extreme fulness of the great veins on the inner surface of the thoracic and abdominal cavities.* The blood on both sides of the heart mostly fluid. Gall bladder moderately full or distended, and contents natural. Bladder empty. The intestines usually filled with the characteristic fluid, and I have met with large masses of scybala in the colon, in a patient dying after copious dejections in the collapse stage.

I do not believe the effusion from the alimentary canal is dependent on a congested state of the internal organs attempting to relieve itself by increased secretion, but that it is (as Dr. Parkes observes) an *exhalation* arising from relaxation and heightened irritability of the intestines, which favour its egress from the vessels, and then eliminate it from the system —not an effort to throw off a poison, but an endeavour of the muscular coat of the bowels to overcome and relieve distension.

With regard to an eruption in cholera, Dr. Roupell states that an eruption has been seen in *some* few instances in this country, and occasionally abroad, and Rayer also mentions a roseola cholerica, which is thus described by Erasmus Wilson: "After the period of reaction, there occurred in some patients (Paris, 1832), especially in women, an eruption, which most generally appeared on the hands and arms, and then extended to the neck, the breast, the belly, and the upper and lower extremities. At its commencement it was characterised by patches, for the most part of an irregular circular shape, of a bright red colour, elevated above the surface, and but slightly itchy. Very numerous on the hands, arms, and chest, they were less on various other parts; in some places they were crowded together, tended to confluence, and had an appearance very analogous to the efflorescence of slight scarlet fever; in other places, the aspect of the eruption was rather like that of measles; and in others, even more like that of urticaria. I have seen this inflammation complicated with an inflammatory affection of the fauces and tonsils, and its disappearance followed by an aggravation of the general symptoms, and sometimes even by death. On the chest the spots occasionally became confluent, and gave rise to patches as broad as the hand, raised above the general level, and pretty well defined. The eruption then acquired a dirty pink, or rose colour. About the sixth or seventh day the epidermis cracked, and was thrown off in large flakes on almost all the places where the eruption had existed." ('Skin Diseases,' page 155.) (See also 'Guy's Hospital Reports' for 1857-8.)

Treatment.

As a prelude, it must be borne in mind that practitioners publish cases successfully treated, without statement of their intensity, or of the period of the epidemic. It is agreed by all that attacks are proportionately much less fatal during its wane.

In the premonitory, or choleraic stage, *before* the advent of algide symptoms, sulphuric acid is, in my opinion, the most valuable medicine; but it becomes less and less useful after incipient collapse. It is perhaps, on the whole, the most reliable of any single agent, and, if serviceable, generally proves so after one or two doses, which should be given at intervals of ten minutes, to the extent of forty minims of the dilute acid in an ounce and a half of water or peppermint. If it fail to arrest the diarrhœa and vomiting, the disease often proves intractable to every remedy. At the same time the patient should be put to bed and well covered, though in the algide stage there is a general tendency to push off the bed-clothes, for, as Dr. Parkes remarks, "cold is more grateful than warmth to the patient." Ice, iced-water, or cold water, may be supplied *ad libitum*, which Professor Maclean also recommends. Cold drinks are generally eagerly desired during collapse, though Dr. Rae, of Newcastle, allowed only a limited supply of fluid, and administered opium and styptic injections. Opium is certainly useless during the collapse stage; but Dr. Parkes speaks in favour of it, in combination with acetate of lead, calomel, creasote, and bleeding. He never found frictions, blisters, or mustard of any service in relieving the algide symptoms, though they might allay spasm; but I have found chloroform give more relief from the cramps. It only causes cessation of pain, without producing any permanent improvement. All patients died who were treated with castor-oil on the eliminant plan by Dr. Filliter and myself; and this treatment was adopted towards the end of the epidemic, at which period a large percentage usually recovers under any plan of treatment. I know nothing of the treatment by sugar which has been advocated. Emetics

of mustard appear occasionally to rouse the patient; but I never observed any ultimate good from their administration. They do not allay the vomiting, which has however yielded, I believe, at times to creasote, capsicum, and calomel combined. Chlorate of potash does no harm, and perhaps no good. Salines seem to exert little action in deep collapse, but are useful in consecutive fever. They were extensively tried in the St. Marylebone Infirmary, after the formula of Dr. Stevens; but I have never seen the effect of saline injections into the vessels. They are good theoretically, but it is unfortunately all exosmosis, and Dr. Parkes relates that injections never saved one patient. Sinapisms, hot-air baths, and other external applications are of little use, and fail to warm the patient. The hot-water bath I have never seen tried, but frictions of the extremities and trunk with the hands, or turpentine or warm fomentations give ease in early stages of collapse, but are useless in its profounder forms. Large doses of chloric ether and ammonia have now and then a temporary beneficial effect. This treatment roused a woman apparently dead, but although she spoke, she sank within a quarter of an hour. I have never given trial to the sulphur treatment, so strongly recommended by the late Mr. Grove. Quinine in a few cases was of no benefit. Chalk, catechu, and remedies of this class are of course useless.

Calomel, strongly recommended by Dr. Ayre, whatever its *modus operandi*, seems at times to be of service. If not absorbed, it can do no harm, and there is little fear of its absorption causing salivation in the consecutive stage. Of course, it is mostly rejected as soon as swallowed, and no practitioner of intelligence would push its exhibition to extremes. If I have a theory, it is in favour of the mercurial controlling the capillary circulation. Dr. Ayre thought the calomel is converted into grey oxide by the alkalies of the stomach; but I have found the ejecta acid. Dr. Hassall, however, says the rice-water discharges are always, as far as he has observed, highly alkaline.

In the worst cases, perhaps, Dr. Parkes is not far from the truth in asserting that one remedy is not more useful than other; but I cannot agreed with Dr. Parkes, Dr. Johnson,

and Mr. Sedgwick that bleeding can be of service, as the state in cholera is vastly different—altogether dissimilar—from conditions such as pneumonia, asthma, bronchitis, and other forms of apnœa, in which there is a loaded state of the right heart, a full venous system, and a mechanical impediment to the circulation through the lungs, so that carbonic acid accumulates in the blood. In cholera, on the contrary, there is arrest of oxidation, nonformation of carbonic acid, little embarrassment of the respiration, and no dyspnœal symptoms, as the patient almost invariably lies recumbent on one side, whilst in the former diseases there is alarming orthopnœa or coma.

In future epidemics, carbolic acid and the carbolates, advocated by Dr. Sansom, deserve a trial as well as the sulphites so strongly spoken of by Dr. Polli, of Milan, and by Mr. Spencer Wells. Yet I have given the hyposulphite of magnesia in the more malignant forms of scarlatina with disappointing results. Much is also to be expected from the hypodermic employment of the alkaloids, especially of atropia and digitaline, or a combination of morphia and atropia in the manner adopted by Brown-Séquard.

Since the foregoing was written, I have to add (without discussing the theory which suggested its adoption) that hypodermic injections of chloral, decimally diluted with water to the extent of two to ten grains, are reported to have been very efficacious in a few (nineteen) sporadic cases in Oudh. This remedy deserves, and will doubtless obtain, a more extended trial.

In addition to drugs, if the vomiting cease, pure iced milk and lime-water in small quantities may be given, if agreeable, to support the patient over the consecutive symptoms. Brandy does no good in collapse, and may prove subsequently harmful, if the patient survive till reaction. It will be gathered from the foregoing outline that the experience of one who had charge of the cholera wards in a parish of 160,000 inhabitants, affords the dismal prospect that the best remedy in incipient cholera is sulphuric acid, and that in the more aggravated forms therapeutics are almost helpless. The most

experienced practitioners are inclined to leave their patients unmolested, with unlimited pure water to allay the thirst, the excretions being received into a vessel disinfected by carbolic acid and sulphate of iron, or chloride of zinc; the symptoms being carefully watched that the access of consecutive fever may be as much as possible mitigated, and the action of the kidneys restored in prevention of uræmia, success in our efforts being much determined according as the renal organs are diseased or sound.

The injunction of the venerable Diemerbroëck, *fuga cum timore Domini copulata est præstantissimum et certissimum prophylacticum à peste,* savours of a bygone age. The true and universal preventive of cholera is to be sought, as Mr. Simon admits, less in quarantine than in national sanitation—that is, in cleanliness, interpreted in its most comprehensive definition.

PRINTED BY J. E. ADLARD, BARTHOLOMEW CLOSE.